

explanation. In the above quoted paper on luminous phenomena I mentioned briefly that ideas of doubles and subtle bodies had been proposed in the past to explain the phenomena in question (Alvarado, 1987, p. 53). I considered the concepts, however, to be unsupported speculations. I must disagree with Vieira when he says that I was dealing with cases of projection of consciousness outside the body. The evidence on the matter does not justify such an assertion except as a tentative hypothesis.

To close, I wish to state that although I disagree with Vieira's methods and conceptualizations, I do think his work is important. I agree that OBE research should consider a variety of factors and explanations other than psychological ones.

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#### To the Editor,

Having read John Palmer's book review of Brian Inglis's 'The Hidden Power' I would like to place on record my own comments regarding Inglis's treatment of the issue of magicians and parapsychology.

As we know, magicians are often self-publicists, jealous of their reputations to an almost paranoid extent, frequently pass scurrilous comments about their fellows behind the victims' backs, and often comment on subjects about which they have no personal knowledge.

Parapsychologists, of course, are free of such vices.

All the same, I am left with a feeling that Dr. Inglis may not have researched as deeply as he might have done.

On page 250, for instance, Inglis writes 'When standing by the grave of an old friend and fellow magician (Houdini) said . . . "Lafayette, give us a sign that you are here" (Presumably a reference to Charles Stentor's "Lafayette, we are here" when American troops landed in Europe in 1917, a tribute to the illustrious Marquis who had served under Washington in the War of Independence).'

In fact, Houdini was referring to Sigmund Neuberger, a magician known as The Great Lafayette, who was a close friend of Houdini. Neuberger was a strange character with few friends and whose only long term companion was a mongrel dog given to him by Houdini.

One would have thought that any discussion of magicians versus psychics would benefit from study of the extensive literature available, such as Corinda's *13 Steps, Confessions of a Medium*, Dunninger's *Inside the Medium's Cabinet* and the like. Inglis demonstrates no knowledge of these books, none are mentioned in his references. Nor does he mention Ben Harris's *Gellerism Revealed* nor *Confessions of a Psychic*, surely essential in any discussion of Geller versus magicians.

Inglis's attack on magicians is based purely on *ad hominem* claims, not on any study of attempts by magicians to replicate psychic effects. Had he attempted such a study, he would have discovered that there have been successful replications: Why, for instance, did he ignore S. J. Davey's work? Surely, at least a passing reference would have been relevant?

There certainly are valid criticisms to be made about the attitudes of one or two of the more vociferous conjurers critical of paranormal claims. Sadly, Dr. Inglis's criticisms appear to be very poorly backed-up.

I have great respect for Dr. Inglis's skills as a polemicist for his particular point of view, but if he is to criticise magicians and suggest, as he seems to, that they have no part to play in psychical research, he must do so from firmer, and better researched ground than he presents in *Hidden Power*.

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Brian Inglis replies,

I did not think anybody could so egregiously miss the point of my Lafayette aside—for which, incidentally, I was indebted to Professor Ian Stevenson. Houdini was of course referring to the magician Lafayette. But 'Lafayette, we are here', used by Stenton as a retrospective thank-you to the statesman for his help in the war of independence, apparently acquired much the same currency in the United States as Pétain's 'Ils ne passeront pas' did in France. Houdini would surely have known it; and what could be more probable than that he had it in mind at the magician's grave?

In *The Hidden Power* I wanted to show how untrustworthy much of the evidence has been which magicians have provided to discredit psychical research. I could have added Mr. Couttie's abysmal collection to mine, but what would have been the use of labouring the point? Of course the phenomena can be and have been faked—as indeed Davey showed; he was in a different category. But my contention was that no conjurer has ever been able to replicate some of the well-attested feats—D. D. Home's, say—in the same conditions.

As for Mr. Couttie's disgraceful assertion that my attack on magicians 'is based purely on *ad hominem* claims', I challenge him to name a single instance where I have used them—though, so help me, in one of the cases it would have been only too easy!

To the Editor,

If Brian Inglis can construe (1) the passages which I quoted from Myers and Sidgwick, and the reference to Podmore, as evidence for Richard Hodgson's fraud against Eusapia Palladino, then he and I do not share the same notion of evidence.

Dr. Harrison misunderstands me when he says (2) that evidential standards have not changed since the 19th century. I was questioning whether Hodgson's handling of the documents he used in evidence, of which Harrison complains in

his original paper (3), was significantly worse than that of his contemporaries, even though it might fall short of present-day procedures.

Harrison complains of Hodgson's selection of evidence; but having quoted Mohini that the third astral appearance of a Mahatma occurred 'about the end of January or the beginning of February', he then proceeds to consider dates only in February. If this is not selection of evidence, what is it? Harrison confidently asserts that Hodgson did not know the date of this occurrence. What evidence has he for this assertion? I would suggest that if Hodgson did not know the date, there would have been no point in referring, as he tells us he did (4) to the calendar. Mohini may not have remembered the date, but this does not preclude Mme. Coulomb from remembering it, and telling it to Hodgson. Harrison's picture of the wicked Dr. Hodgson—perhaps twirling his moustache—sternly confronting the faltering Mohini with the calendar, may be all very well for a Victorian melodrama, but seems out of place in a scientific journal. The unsuspecting reader might suppose from Dr. Harrison's version that Hodgson discredited Mohini's evidence solely on the basis of this moonlight episode. In fact, as the Hodgson report shows (5), his evidence was challenged in respect of the two previous occasions of astral appearances at Adyar by a number of other witnesses; and his evidence concerning the Paris letter (5) was suspicious. His subsequent behaviour in Paris was ultimately to prove an embarrassment to Mme. Blavatsky herself (6).

In his efforts to show that the Blavatsky-Coulomb letters were forgeries, Harrison's account of Dr. Elliott Coues' involvement with Mme Blavatsky and William Q. Judge is misleading. The relevant facts are that in 1888, when he was an official of the American branch of the Theosophical Society, Mme. Blavatsky had fallen out with Coues, because he had had the temerity to produce a Mahatma letter without her mediation, and had published a denial that Koot Hoomi had dictated '*Light on the Path*', with Mabel Collins' statement that Mme. Blavatsky had begged her to lie to this effect. Mme. Blavatsky had succeeded in having Coues expelled from the Theosophical Society in 1889; and in 1890 he had published a bitter attack on the leaders of the Society in the *New York Sun*, with particular emphasis on Col. Olcott, Judge and Mme. Blavatsky as the principle miscreants. His attack on Mme. Blavatsky included a statement that she had been the mistress of Prince Emile de Wittgenstein, and had borne him a crippled son (7). It was this last statement that formed the basis of a libel action which Mme. Blavatsky brought against Coues, but which never came to court, because (under the laws of New York state) the action automatically lapsed with the death of Mme. Blavatsky in 1891. However Judge brought a separate action against Coues and the *Sun*, and it was in settlement of this action that the *Sun* published a retraction in the following year (8). Clearly neither the subject of Mme. Blavatsky's action, nor the retraction secured by Judge, can have any bearing on the authenticity of the Blavatsky-Coulomb letters. Dr. Harrison emphasises the high price which Coues paid for these letters; but this must argue for their genuineness, since no-one of Coues' undoubted intelligence would have paid a high price for worthless forgeries. Since the present whereabouts of the letters is unknown, no inferences can be drawn from presumptions about their supposed treatment. In referring to the high price which Coues paid for the letters, Harrison makes no mention of the fact that, as the result of an

advantageous marriage, Coues was practically a millionaire. Dr. Harrison, following my reminder that the Madras Christian College (the then owners) had placed restrictions on Hodgson's use of the letters, claims that the S.P.R. should not have made use of them, since they were not available to Mme. Blavatsky's defenders. This is unreasonable on two counts, firstly because incriminating portions of the letters had already been made public (9), and Hodgson could clearly not ignore them; secondly, Madame's defenders did have the opportunity of examining the letters, but only a few availed themselves of this (10).

I did not claim in my letter (11) that the peculiarities shared by Mme. Blavatsky's writings and the K. H. letters 'proved' her authorship of both. I merely pointed out that in what was represented as a critical review, the topic was not even mentioned.

Nowhere did I suggest that the early members of our Society were infallible: I was arguing against Brian Inglis' suggestion that they had accepted Hodgson's verdict without examining the evidence for themselves, since this would be at variance with what we know of their characters.

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9. Patterson, D. (Editor) *The Collapse of Koot Hoomi I & II*. Christian College Magazine, (Madras), 1884 September & October.
10. *The Collapse of Koot Hoomi*. Madras, Christian Literature Society, 1904, pp. 56-7.
11. Coleman, M. H. *JSPR*, 1987, **54**, 158-9.

To the Editor,

Some eleven years ago the late Dr. Dingwall wrote to me as follows: 'An account of the Cambridge sittings [with Palladino] with full transcripts of the MS records together with an introduction would be *very interesting* (my ital.), but I don't know whether anybody would be willing to do it.' (1).

I had in fact already suggested it. Dingwall himself had briefly dealt with the subject, saying, 'What actually happened at Cambridge we shall never know. The full and detailed reports have not been published, but remain buried in the archives of the Society for Psychical Research.' (2).

A complete transcript etc. has been for several years in the Library of our Society where it could have been consulted by Mr. Coleman who, for reasons best known to himself, has chosen to ignore it. It is the kind of work that before the

escalation of printing costs might conceivably have been published as a Proceedings, but at a more realistic level one had to be content to compress the conclusions of years of hard labour in the form of a brief article in the Journal. It duly appeared in February 1983 (Vol. 52, No. 793), and I now merely refer those interested to my 'Palladino at Cambridge'. It does not altogether redound to the greater glory of the Founders, least of all of Hodgson.

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2. Dingwall, E. J., *Very Peculiar People*. Rider n.d.

To the Editor,

I regret the need to put to rest the latest example of misinterpretations of what are now known as SORRAT phenomena. (These are quite extensive, varied and lengthy, and have commanded my own research attention for over a decade.) Dr. John Palmer, in his review has criticized Dr. Brian Inglis for not having cited in his *The Hidden Power* 'the evidence for fraud uncovered by Hansen and Broughton.' (p. 153).

Dr. Broughton himself responded to my criticism of a review of *The Paranormal* by D. S. Rogo with an opinion that there had been planned fraud, and with doubts that I was ever the victim of a mail thief (as I contended, on good evidence, was the case).

The actual details of Hansen and Broughton's experiences were, logically in my opinion, omitted from the book because they are not clear indications of fraud. The latter is simply a conceivable alternate explanation under the diverse circumstances that prevailed.

My chief complaint is therefore in their singling out this disquieting deviation despite their being aware of my numerous other unprecedented SORRAT documentations.

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To the Editor,

Monte Carlo methods are becoming more frequently used in parapsychology. John Palmer (Edge, Morris, Palmer, Rush, 1986) indicated that such methods 'may well be the wave of the future in evaluating psi data' (page 151). One of the first examples was presented by Michael Thalbourne in an exceptionally readable article 'A More Powerful Method of Evaluating Data From Free-Response Experiments' (*JSPR*, 50, 1979, pp. 84-107). Unfortunately, that paper contains a number of serious errors and misconceptions.

1. The description of the Randomization Test given on page 92 is incorrect; the method described counts combinations that cannot occur with the actual experimental procedure. If the described method were used it would result in a wrong probability value being estimated. [Also, the number of combinations of

100 items selected 10 at a time is approximately  $1.73 \times 10^{13}$  and not the value given by Thalbourne on page 92.]

2. Thalbourne's actual RANMAT program does not fit the description given in his Journal article.

3. The actual RANMAT program is essentially a Monte Carlo implementation of the 'direct-count-of-permutations' method described by Schlitz and Gruber (1980). This assumes a closed deck condition with nonindependence of trials. The implemented RANMAT method is not completely comparable to 'Morris' Exact Test' (Thalbourne's term; sometimes referred to as a sum-of-ranks statistic) because Morris' Exact Test assumes independent trials (Morris, 1972; Solfvin, Kelly, and Burdick, 1978).

4. Thalbourne claims that his Randomization Test gives greater statistical power over that of Morris' Exact Test. He gives no real evidence for this claim.

5. Thalbourne states 'The reason for the greater power of the Randomization Test [over that of Morris' Exact Test] is basically that it is "distribution free"' (p. 93). Morris' Exact Test is also 'distribution free'.

6. Thalbourne gives no confidence limits so that one might estimate accuracy of his simulations. Without this, we do not have any assurance that a claimed significant effect is indeed significant.

These points were brought to the attention of Dr. Thalbourne in 1982.

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Solfvin, G. F., Kelly, E. F. & Burdick, D. S. (1978). Some new methods of analysis for preferential-ranking data. *JASPR*, **72**, 93-109.

Michael Thalbourne replies:

The vast potential for the use of Monte Carlo methods has not been realized even in statistics, let alone in psychical research. It is therefore a pleasure to see a parapsychologist go to some trouble, as George Hansen<sup>1</sup> recently did, in singing the praises of this useful analytical approach.

However, Mr. Hansen's letter (this journal) compels me to say that, regrettably, its author has been prosecuting his interest in a way which, it seems to me, departs from the standards we expect of professionals. But readers will be in a better position to judge for themselves if I fill them in on some of the facts surrounding Mr. Hansen's letter.

I last spoke with Mr. Hansen at the meeting of the Parapsychological Association which was held in August 1986 at Sonoma State University, California. Mr. Hansen was presenting, at a poster session, the above-cited paper on Monte Carlo methods. I read it with interest, but was surprised to find

that my own 1979 paper<sup>2</sup> on the topic was not even mentioned, though Mr. Hansen knew it very well. Mr. Hansen justified its exclusion on the grounds that it contained "major" errors, a correction of which I had "still not bothered to publish". This attitude rather puzzled me: it is surely professional courtesy to cite previous relevant work, and, even if it allegedly contains some error, the *scholarly* approach would be to cite the work *and* point out that error. In that way, one makes a name for oneself as a skilful reviewer. It is odd that Mr. Hansen chose to forgo such an opportunity.

Not long after the Convention, Mr. Hansen wrote and sent his letter to the editor. He also sent a copy of it to Dr. John Palmer of the Institute for Parapsychology (who had made some laudatory remarks about my 1979 paper in a recent textbook of parapsychology<sup>3</sup>), without at the same time doing me the professional courtesy of sending a copy to *me*, at whom, of course, it was directed. (I owe the copy that I have to the Editor of the *Journal*.) But more to the point, I can only wonder why Mr. Hansen neglected to say in his letter that information effectively answering the points he raises has been available since 1981, when it was included in Chapter 2 of my doctoral dissertation<sup>4</sup>—a document which he himself had read very closely! The first and second of the Hansen points—which are, in fact, different facets of a single error which I made in my description—were answered on pp. 75–76 of my thesis. Point number 6 was answered on pp. 77–78. Evidence relevant to point number 4 was presented in Table 1 of my 1979 paper, though, as point number 3 correctly states, results from my Randomization Test and Morris' Exact Test are not comparable; yet I pointed this fact out myself in 1981 in a footnote to p. 73 of my dissertation! Therefore, the statement that "These points were brought to the attention of Dr. Thalbourne in 1982" appears to be suspect on chronological grounds.

Nevertheless, since there has been some interest recently in my 1979 paper, it is appropriate that the corrections of it which are detailed in my dissertation be made available to a wider public than has hitherto been reached. Therefore, along with this letter, I have submitted for future publication a Technical Note.

As for Mr. Hansen's tendency to amnesia, let us hope that professional behaviour in parapsychology is something we *can* rely on—unlike our chances of success at the tables of Monte Carlo!

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